Contributions

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To Work or Not to Work? The Effect of Childcare Subsidies on the Labour Supply of Parents

Abstract: This paper studies the effect of childcare subsidies on parental labour supply. I use variation arising from changes in the municipality-specific supplement to Finnish child homecare allowance to identify the causal effect of subsidies on the labour force participation of parents. The variation in labour supply incentives is plausibly exogenous, since eligibility depends on municipal-level rules, but not on family income. Robustness checks indicate that the results are not driven by policy endogeneity or residential sorting. I find a robust result that 100 euros higher supplement per month reduces the maternal labour supply by 3 percentage points.

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JEL Classification: J22, J13, H22

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1 Introduction

In designing childcare policies, policy-makers should know how these policies affect parental labour supply. Lower childcare prices sometimes have a positive impact on maternal employment (see e.g. Gelbach 2002 and Baker, Gruber, and Milligan 2008). However, in the Nordic countries, where low childcare prices ought to lead to high maternal employment (OECD 2009), childcare prices seem to have a rather minor impact on maternal labour supply (Lundin, Mörk, and Öckert 2008 and Simonsen 2010). Moreover, it may be that homecare subsidies have a different impact than other childcare subsidies, since they more directly affect parents' financial gain from participating in the labour

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market (Schone 2004). Therefore, more evidence is needed on the labour supply impact of different childcare policies to back up policy considerations.

This paper studies the Finnish child homecare allowance (HCA), which has several interesting features. The HCA reduces labour supply incentives, since it is related to staying at home. It is more generous than homecare-related subsidies elsewhere (Sweden, Norway (Schone 2004 and Naz 2004) or Thuringen, East Germany (Gathmann and Sass 2012)), which makes it a real alternative for even high-income parents to stay outside of the labour force for prolonged periods. A mother can stay outside of the labour force for several years on benefits from 300 to 700 euros per month. Moreover, the HCA is provided in the context of Nordic childcare institutions, where heavily subsidized public daycare is readily available.

It is challenging to estimate the impact of childcare prices or the HCA on parental labour supply. The fundamental problem is that parents differ in other aspects than the amount of HCA they are eligible for. In particular, parents may have different abilities to generate income in the labour market. This is unobserved to the researcher, but is correlated with the daycare fees and the HCA, which are income dependent. Thus a regression of the HCA on parental labour supply would produce a spurious result.

This paper overcomes this challenge by utilizing changes in the amount of the HCA to cleanly identify its impact on parental labour force participation. The variation comes from municipalities providing supplements to the HCA. Importantly for identification, the supplements do not depend on family income and there are reforms to supplement rules. The supplements are typically a fixed amount that depends on the age of the youngest child and the number of children in the family and are conditional on the child being in homecare. The supplements range from non-existent to more than 200 euros per month.

Reforms to municipal supplements provide a good case of a regional experimental set-up. The reforms take place at different points in time, making the results robust against unobserved macroshocks to parental labour supply, a potential problem in many previous studies (e.g. Baker, Gruber, and Milligan 2008; Baker and Milligan 2010; Gathmann and Sass 2012 and Schone 2004). There remains a worry that supplement policies are endogenous reactions to changes in macroeconomic conditions or political preferences. I am able to show that the treatment and control group parental employment trends follow each other before changes in a supplement, as well as other robustness checks that indicate that these policy endogeneity issues do not invalidate the results. Supplement policies differ in size and child-age cut-off rules. With this rich variation, I am able to estimate the labour supply elasticity of the allowance, not just the average treatment effect of giving a fixed amount of subsidy.

The data in this study cover income register information for a sample of households. The variables in the data include earned income, demographic characteristics and information about family structure. I utilize an earned income-based employment measure. Moreover, I observe the national daycare fee, the HCA and municipal supplement rules. The rules depend on characteristics such as the residential municipality, the age of the youngest child and the number of children in the family, which are all observed in the microdata. Utilizing this, I calculate the amount of supplements everyone is eligible for. This eligibility amount is used as an independent variable in the analysis. It is exogenous to labour supply choices, since it does not depend on income or employment status, rather it depends on the residential municipality and the age of the youngest child.

The results indicate that mothers strongly reduce their participation in the labour market in response to the financial incentives created by the HCA. Three percentage points fewer mothers participate in the labour force if the homecare allowance is increased by 100 euros. Interestingly, fathers are also eligible for this policy, but they do not respond to the HCA. The HCA is at such a high level that mothers on an average income can afford to stay at home for prolonged periods. Interestingly, a divided sample result indicates that highly educated mothers respond strongly to the HCA, even more strongly than medium-educated mothers. The main results survive a battery of robustness checks, indicating that they are not driven by policy endogeneity or differing labour supply trends.

This study relates closely to earlier research on homecare-related childcare subsidies. Schone (2004) and Naz (2004) study the Norwegian Cash-For-Care (CFC) policy. The difference between the Finnish homecare allowance and the Norwegian CFC is that the former is only for homecare, whereas the latter is for all non-public childcare. The incentives not to supply labour are stronger in the former. Thus it is not surprising that the labour supply response found by Schone (2004) and Naz (2004) is smaller than that found here. Gathmann and Sass (2012) analyse a homecare subsidy scheme in Thuringia, East Germany. They find strong maternal labour supply responses, similar to this study. They find the strongest effect for low-income families, which could be due to the low level of total homecare subsidies available. Lastly, Gonzalez (2013) studies a Spanish policy, where support is provided to families with small children. This policy is different from the HCA, since it is not conditional on labour supply. Despite this, the policy seems to create a labour supply response, which could be due to an income effect. The subsidy allows mothers of young babies to stay outside of labour force for a few months.

This paper contributes by estimating how elastic the participation decisions are to homecare subsidies. I calculate the impact of the financial gain from

participation on the participation decision in an IV regression. Municipal supplements are the instruments in this estimation. This produces an internally valid measure of participation elasticity, which is also of interest for other countries. The estimated participation elasticity is of interest for other countries, since it utilizes exogenous variation in financial incentives keeping institutions constant. Of course the size of the elasticity might differ in different institutional contexts. It is possible to estimate the participation elasticity in this manner because of the rich variation in the amount of the HCA a parent is eligible for that is not related to family income. Robustness checks indicate that supplement policies are not endogenous to labour market conditions. The results imply a participation elasticity of 0.8, which is relatively high but within the bounds of elasticities found in earlier literature.

The majority of the literature on the impact of childcare institutions focuses on policies that increase labour supply incentives for parents. Lower childcare prices increase the maternal labour supply (see e.g. Gelbach 2002 and Baker, Gruber, and Milligan 2008), provided maternal employment is not at a very high level to begin with (Lundin, Mörk, and Öckert 2008; Simonsen 2010 and Havnes and Mogstad 2011). Furthermore, many programmes are targeted at the working poor or low-income families, like the Earned Income Tax Credit policy in the US, the Canadian National Child Benefit and the Working Families Tax Credit in the UK. These policies seem to increase the labour supply of single women in particular (Eissa and Liebman 1996 and Eissa and Hoynes 2004; Milligan and Stabile 2007 and Blundell et al. 2000 and Brewer et al. 2006).

Another strand of related literature studies the expansion of childcare and pre-school programmes that implicitly reduce the price of childcare. The evidence from them is mixed. Havnes and Mogstad (2011) find no impact on parents' participation in the labour force from an expansion of publicly provided childcare. Moreover, Fitzpatrick (2010) finds little employment effect from free pre-school kindergarten programmes. Instead, Gelbach (2002) and Cascio (2009) find that expanding free school programmes to 5-year olds has an impact on the maternal labour supply. Goux and Maurin (2010) find that public schooling for 2- and 3-year olds increases the labour supply of single earner families. Similarly, Nollenberger and Rodríguez-Planas (2011) find that universal childcare for 3-year olds increases maternal labour supply and that these effects persist over time. Also Felfe, Lechner, and Thiemann (2013) find that after-school provision of childcare increases maternal full-time employment while reduces paternal employment.

The rest of the paper proceeds as follows. Section 2 presents the source of variation in childcare subsidies and a short description of the Finnish childcare system. The identification issues and the econometric specification are discussed

in Section 3. Section 4 describes the data. The estimation results and some robustness checks are given in Section 5. Section 6 presents the estimation of participation elasticity. The last section concludes the study. The Appendix presents further robustness checks.

2 Forms of childcare

This section first describes Finnish childcare institutions and the financial incentives they create. After that the section describes the variation in incentives created by municipal supplements and how municipalities provide them.

2.1 Finnish childcare institutions

The Finnish childcare system provides financial assistance to parents starting from the birth of a child. Most mothers take up a generous maternity leave, which ends when the youngest child is 9 or 10 months old. After the maternity leave, parents can continue to take care of the child themselves. When a young child is cared for by a parent, he or she is entitled to the HCA. All children who are not in public or private daycare and are between 9 months and 3 years old are eligible for this allowance. The HCA may be paid to either parent, although it is predominantly the mother who takes up the allowance.

The amount a family is eligible for depends on the family's characteristics and ranges from 300 to 700 euros per month. There is a fixed amount of 255–315 euros per month (depending on the year), which does not depend on income. There is a means-tested part targeted at medium- to low-income families, not exceeding 180 euros per month. Additionally there is a sibling extra, which is from 60 to 100 euros per month per sibling cared for at home. On top of these allowances, some municipalities provide supplements to the HCA.

If a parent receives the HCA, she or he may not receive other forms of childcare support (public or private daycare) for the same child. This feature rules out the use of the HCA for financing private daycare.[1] A parent taking up the HCA can, after the HCA period ends, return to the same job that he or she left.

If parents choose not to take care of their children themselves, they can either place their children in public or private daycare. Both child daycare options are subsidized by the government.

1 This is subsidized from a different allowance, the private daycare allowance, and is more attractive for the third party (the carer).

Public daycare is the predominant choice of daycare in Finland, especially for children aged 3 years or above. Every child under the age of 7 (when they start primary school) is entitled to a public-daycare place if their parents request it.[2] A child can be placed in public daycare even if neither of the parents is employed. Daycare fees are subsidized by the government. This means that parents pay only a part of the total costs of the public daycare in fees. The daycare fee schedule depends on family income, and there is a cap on daycare fees of (less than) 200 euros per month. The schedule is such that even families earning below the medium income pay the maximum fee. In 2005 a typical family with two children paid 380 euros per month for a place in public daycare. The law controls the quality of public daycare, for instance the minimum number of nurses per child. Childcare nurses are highly educated, and daycare centres also provide pre-school education, especially for older children.

Private daycare is subsidized by the private daycare allowance.[3] Furthermore, municipalities provide a municipal supplement to the private daycare allowance. Even with these allowances and supplements, in the majority of cases private daycare is more expensive than public daycare. Thus private daycare has not been very popular in Finland.

2.2 Municipal supplements and reforms to them

The municipal supplement to the HCA constitutes the interesting variation in this study. Some municipalities pay a supplement on top of the national HCA while other municipalities have no supplement policy. The most important features of these supplements for the identification is that they do not depend on family income and that municipalities reform their supplement policies.[4] The supplement is typically a fixed amount that depends on the age of the youngest child and the number of children in the family and on the condition that the child is cared for at home. Thus the supplement increases the incentives to stay outside of the labour force. The amount of the supplement and the child-age threshold vary by municipality. Moreover, some municipalities have a prior work condition, which requires that the eligible parent had a job prior to having the child.

2 This is stated in legislation. Before 1995 the law stated that every child under the age of 4 is entitled to a place in public daycare.

3 This system has been in place nationwide since 1997. Between 1995 and 1997 there was an experiment in 33 municipalities that provided a similar allowance. Viitanen (2007) found a positive effect on the use of private daycare, but little effect on labour force participation.

4 In some rare cases the supplement does depend on family income. Excluding such municipalities does not change the results, and these cases are not part of the main analysis.

The variation in financial incentives in this study comes from municipalities changing their supplement rules. This allows me to compare similar mothers before and after such changes in incentives take place. There are different kinds of changes. In the majority of cases the change is to implement a supplement policy for the first time. There are also some cases where municipalities have ended their supplement policy. Other kinds of changes include changes in the amount of supplement and changes in the child-age thresholds. Municipalities have also changed other eligibility rules, such as ending or introducing a prior work condition.

The extent of variation in financial incentives that the municipal supplements create naturally depends on their generosity. A typical supplement is under 200 euros per youngest child per month plus a sibling extra of 50 euros per month (provided there are older siblings). Thus the typical variation in incentives is that a mother either receives no supplement or receives a supplement of 100–250 euros on top of the HCA if she stays at home taking care of her children. Another typical variation is created by municipality extending the supplement to cover mothers of 2-year-old children.

The eligibility rules for municipal supplements are municipality-specific. In other words, all those living in a municipality who fulfil the eligibility criteria are eligible for the supplement. I observe the eligibility rules and have corresponding information in the microdata (like the age of the youngest child and older siblings). Thus I code into the data a variable indicating what amount of supplement (if any) a mother is eligible for. This coded variable will be the variable of interest in the regression analysis.

The municipal supplement system has been part of the Finnish childcare system since the 1980s. In this study, the observation period stretches from 1995 to 2005. Over this period, there were around 450 municipalities in Finland. Five of them had adopted a supplement policy in 1995 and the figure had increased to 65 by 2005. Figure 1 shows how the municipal supplement has varied over time.[5]

3 Identification and econometric strategy

To estimate the effect of the homecare allowance (HCA) on parental labour supply outcomes, I apply a natural experimental approach. Changes in the municipal supplement eligibility rules for the HCA are the naturally occurring

5 There are cities in both supplement and no-supplement municipalities. However, supplement municipalities are on average more populous.

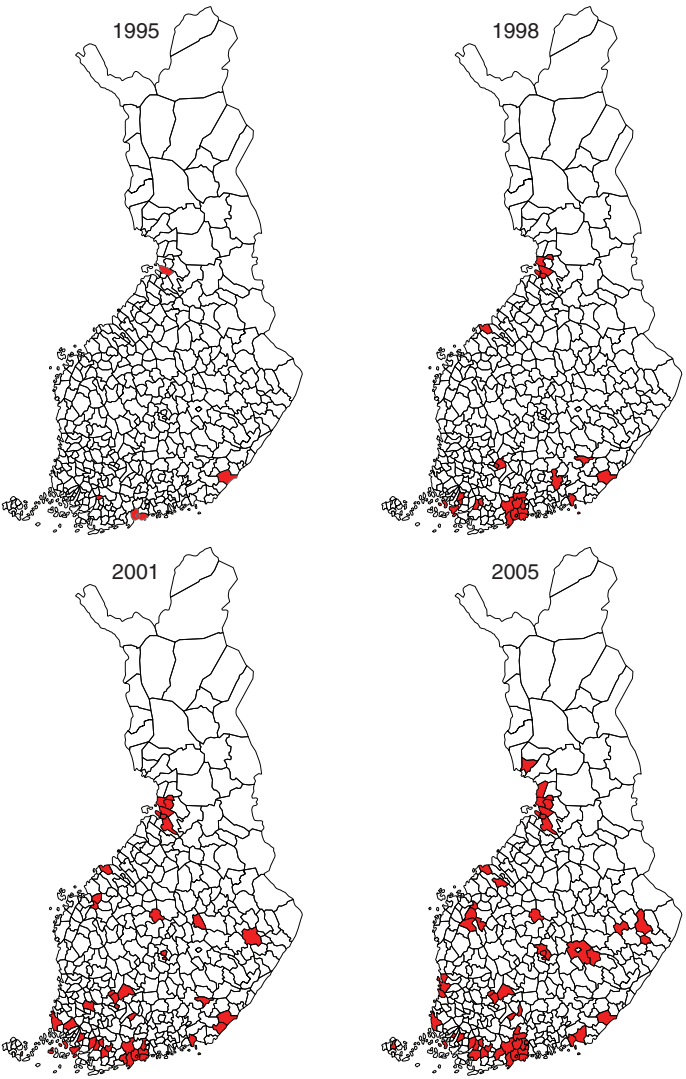

Figure 1: Maps of Finland showing those municipalities with a supplement policy in the years 1995, 1998, 2001 and 2005

variation. Thus I estimate a difference-in-differences (DD) model with continuous treatment (changes in the amount recipients are eligible for). The idea is to compare eligible and non-eligible mothers before and after changes in eligibility rules. I utilize the DD approach, since it is a transparent method that shows the effect of the HCA on the labour supply of parents.

Furthermore, I utilize a triple-difference (DDD) approach in some parts of the analysis. In the DDD estimation the third difference is whether or not the youngest child is under 3 years old. This distinction is meaningful since a child is no longer eligible for supplement from the age of 3. I utilize the DDD approach to control flexibly for regional time trends.

I estimate the OLS equation:

$$Y_{iym} = \alpha + \beta_1 P_{iym} + \beta_2 X_{iym} + \beta_3 Mun_m + \beta_4 Year_y + \varepsilon_{iym} \quad [1]$$

where the dependent variable is either the labour supply indicator or earned income in euros, Y. For labour supply, Y takes a value of 1 when a parent participates and zero otherwise. The key explanatory variable is eligibility for P_{iym} euros of municipal supplement in year y, municipality m and for individual i. The other variables in eq. [1] are the municipal (Mun_m) and year ($Year_y$) dummies, a control vector and a residual error term ε_{iym}.

The core issue for performing the estimation is to identify the effect of the HCA on the labour outcomes of parents. The estimation achieves a causal identification provided that the standard DD assumption holds: the treatment and control groups should behave in similar way over time in the absence of the treatment. Also the selection into treatment should be exogenous to outcome. In mathematical terms, the model should identify β_1, the effect of the municipal supplement on labour supply or income Y_{iym} conditional on controls provided that the following holds:

$$E[Y^0_{iym}|Mun_m, Year_y, P_{iym}, X_{iym}] = E[Y^0_{iym}|Mun_m, Year_y, X_{iym}] = Mun_m + Year_y + \beta_2 X_{iym} \quad [2]$$

where I note the outcome in the counterfactual state (represented by a control group) by Y^0_{iym}. The assumption that guarantees the identification is that P_{iym} (eligibility for supplement) is exogenous to Y^0_{iym} (labour supply). Eq. [2] explains in a mathematical notation that the expected outcome of the control group is the same regardless of whether one conditions on eligibility for supplement or not. If this condition holds, the supplement policy is exogenous in eq. [1]. Then the estimation can be inferred as being the effects of supplement policies on the outcome variables.

The variable P in eq. [1] is the eligibility for x euros of supplement. It is coded in the data for each individual based on the eligibility rules of the supplement. As explained in Section 2, eligibility depends on the municipality, on the age of the youngest child and on the number of siblings. The variable P is the amount (in euros) each parent is eligible for according to these criteria. It takes the value of zero if the parent is not eligible for the municipal supplement. The key for identification, and for the condition in eq. [2] to hold, is that the

value of P does not depend on the labour supply choices a parent makes. The value only depends on predetermined characteristics, like the number and age of children and the municipality a parent lives in.

The identification is strengthened by the fact that changes take place in the amount of supplement each parent is eligible for. Thus it is possible to compare situations where the amount a parent is eligible for is, for example, increased from 100 euros per month to 200 euros per month. The data set I am using does not have enough observations to utilize this variation in independent variable in divided sample analysis. Nevertheless, this gives richer variation in the independent variable than just comparing whether or not a municipality implements a supplement policy. For identification this means that in the control group there are parents living in municipalities with no supplement policy, but also parents in municipalities with a supplement policy where no changes are made in the amount of the supplement. In eq. [2] the municipal-level fixed effects Mun_m take into account the time-invariant factors. After conditioning on these fixed effects, the counterfactual outcome represented by parents in municipalities with no changes in their supplement policies should be unaffected by non-changing supplement policies.

Changes to the municipal supplements occur at different baseline levels of total homecare benefits received. This does not cause a direct problem for identification, since the different amounts received are accounted in the estimation in the independent variable. However, different baseline levels of homecare benefits could create different point estimates, the marginal effect from different starting levels could differ. We would still identify the effect of homecare subsidies on labour supply, but the external validity would be compromised. This problem is limited by the fact that everyone receives the national homecare allowance, thus the total amount of homecare benefits one receives are never zero and we only have smaller variations from this mean.

It follows from eq. [2] that the counterfactual labour outcomes should not depend on the municipal-level supplement rules. This would not hold if there is selection into treatment. As already explained, the most direct selection mechanism does not invalidate the approach, since treatment status does not directly depend on the outcome variables, but on the predetermined or more permanent characteristics of an individual. It remains to be considered, whether individuals might anticipate policy changes and influence their permanent characteristics to benefit from more generous supplement policies. Examples include increased fertility and migrating to a municipality that provides (more) supplement. The fertility issue does not cause great concern in the present case, since it takes a long time from starting to conceive a child to the child being at least 9 months old. The estimation takes into account immediate changes in

labour supply when the supplement rules change. Nevertheless, to check against this concern, I look at the impact of changes in municipal supplements on those who will have a child the next year. This should reveal whether there is any anticipatory fertility effect.

In order to enjoy more generous supplements, it would be easier for parents to move to a different municipality than to conceive another child. Thus migration poses a more relevant concern for identification than fertility. What limits this concern is that the costs of moving to a different municipality could easily exceed the benefits from moving in terms of additional supplements. To check against this relevant worry, I perform estimation where the outcome is an indicator for being a child under 3 years of age. Having children in this age group triggers eligibility for the supplement, provided a municipality provides it. I regress an otherwise similar regression as in eq. [1], but for a population of children in the data. The coefficient for P in this regression should be positive if the increased supplement is associated with an increase in the number of children under the age of three relative to children in other age groups. It would indicate that higher supplements induced families to migrate. As an additional robustness check, I present below regressions with municipal-level data on whether the implementation of supplements is correlated with migration flows to the municipality.

I attempt to identify a causal effect of the HCA on labour market outcomes conditional on control vector X. This conditions on individual-level observables, such as a dummy for each 3 months of age of the youngest child and a dummy for each sibling, since these partly determine the supplement rules. Other individual-level controls are husbands' income, age, age squared and dummies for the level of education.

I also condition on municipal-level variables, since eq. [2] requires that municipalities do not implement supplements in response to changes in employment trends, which would make the policy endogenous (Card and Levine 2000 and Lalive and Zweimuller 2004). The municipal-level control variables should pick up the influence of such changes. X includes the municipal unemployment rate, the total number of women and the costs of daycare in a municipality. Moreover, the counterfactual time trend of the treatment group should coincide with the time trend of the control group. In a sensitivity check, I include a linear time trend for the treatment group and municipality by year indicators in the DDD approach in the control vector X_{iym} without any significant effect on the results. The assumption of parallel time trends between the treatment and control groups is also tested by estimating the DD regression for a group of mothers with older children that are no longer eligible for the supplement.

As a further check against policy endogeneity I estimate equations where the independent variables are the leads and lags of either P (euros of supplement) or a dummy for implementing the supplement for the first time. The equation for the latter is shown below. The leads and lags pick up changes in the outcome before and after a change in supplement policy. It is important that there are no changes in the outcome of those groups that are subsequently treated before any change in the policy rules. Such changes would suggest reversed causality that the rules changed because of a change in the outcome and not *vice versa*.

$$Y_{tm} = \alpha_m + \beta_{-2}1(implem_{t-2,m}) + \beta_{-1}1(implem_{t-1,m}) + \beta_0 1(implem_{t,m}) + \beta_1 1(implem_{t+1,m}) + \beta_2 1(implem_{t+2,m}) + X'_{tm}\zeta + \varepsilon_{tm}$$

I estimate the above equation for variables that potentially indicate that a macroshock occurred in municipal-level data. Figure 2 and Table 1 present the

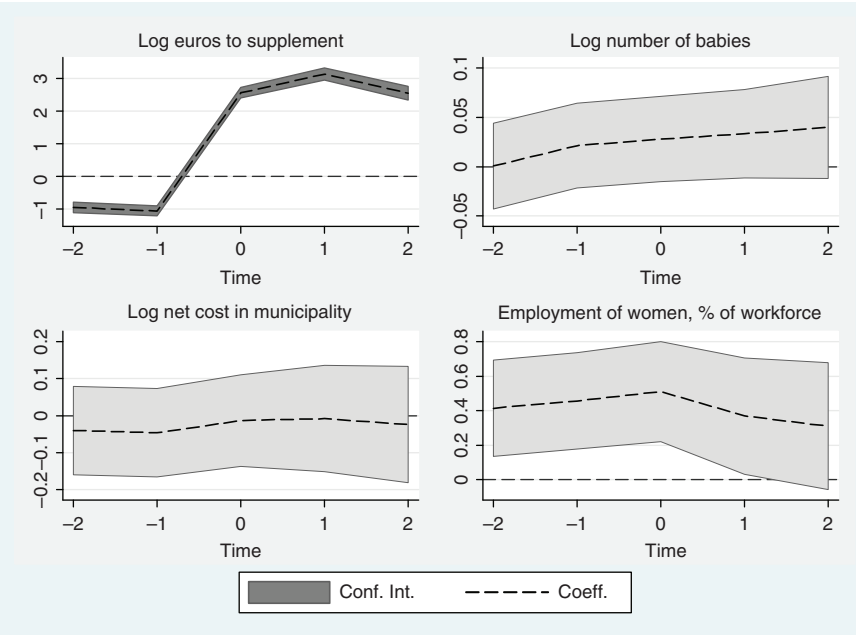

Figure 2: Leads and lags of indicator of implementation of supplement to homecare allowance Notes: The first dependent variable, log euros to supplement, describes take-up of the policy, the next three are background variables: log number of babies describes fertility, log net costs relates to public sector expenses and employment of women shows how good the labour markets are for women in a municipality.

Table 1: Leads and lags of municipality implementing the supplement

Vars.	(1)	(2)	(3)	(4)	(5)	(6)
	Supplem.	**Care cost**	**Priv. care**	**Homecare**	**Empl.**	**Migrate**
implem − 2	−0.95***	0.017	−0.068	0.0033	0.0065**	−0.005
	(0.08)	(0.022)	(0.069)	(0.013)	(0.0033)	(0.028)
implem − 1	−1.06***	0.018	−0.103	−0.0084	0.0069**	−0.015
	(0.08)	(0.022)	(0.069)	(0.013)	(0.0033)	(0.031)
implem	2.56***	0.005	−0.07	6.6e−05	0.0109***	−0.023
	(0.08)	(0.021)	(0.07)	(0.013)	(0.0034)	(0.036)
implem + 1	3.14***	−0.004	−0.162**	0.0244*	0.0076*	−0.021
	(0.1)	(0.022)	(0.079)	(0.0145)	(0.004)	(0.036)
implem + 2	2.54***	0.014	−0.039	0.0182	0.0066	−0.042
	(0.11)	(0.025)	(0.086)	(0.0157)	(0.0044)	(0.041)
N	4,057	4,173	3,406	3,406	4,494	3,430
R^2	0.49	0.18	0.06	0.64	0.8	0.26
N of *Mun.*	290	321	319	319	321	312

Notes: The outcome variable in column (1) is the amount of supplements each municipality paid in total, in column (2) the log of childcare costs per child, in column (3) the log of the number of children in private daycare, in column (4) the log of the number of children receiving the HCA, in column (5) the employment rate and in column (6) the log of migration to the municipality. The independent variables are the leads and lags of an indicator variable indicating the implementation of the municipal supplement.

coefficients from a fixed-effects regression for two leads and lags of the implementation variable.

The data for these estimations come from the financial statements of municipalities and other municipal-level statistics compiled by Statistics Finland. The first estimations in Figure 2 and Table 1 are the amount of resources in euros municipalities use for the supplement. This checks that take-up occurs when supplement policies are implemented, and not before. In addition, Figure 2 shows the results for three other variables that are relevant from the policy endogeneity point of view: the number of babies (aged 0 or 1 years), the net total costs of public sector activities and the employment rate of women in the municipality. There is no significant variation in the coefficients of the leads and lags of the implementation variable. Stable coefficients indicate that the implementation of the supplement is not a response to a change in economic conditions.

Table 1 shows similar results for other outcomes: in column (2) the cost of childcare to the municipality in log euros per child, in column (3) the logarithm of the number of children in private care, in column (4) the logarithm of the

number of children receiving the homecare allowance, in column (5) the employment rate and in column (6) the logarithm of migration to a municipality. Since there does not seem to be significant variation in the coefficients prior to the implementation of the supplement, policy endogeneity does not seem to be driving the results. Moreover it is expected that after the implementation of the supplement, the take-up of the homecare allowance will increase and the use of private daycare declines, as shown by the coefficients.

Inconsistent standard errors are a potential problem in the natural experimental set-ups. Bertrand, Duflo, and Mullainathan (2004) show that potential problems with inconsistent standard errors are less severe if there are many treated and control groups and the reforms are implemented at different points of time. One virtue of analysing reforms in municipal supplements is that there are over 400 municipalities, 65 of which had supplement policies in 2005, contributing to multiple treatment groups. In addition, the reforms were implemented at different points of time. To further minimize these problems, I use robust standard errors throughout clustered at the municipal level.

4 Data and descriptive statistics

The main data set in this study is individual-level microdata for the years 1994–2005. The data come from multiple sources. The base data, Income Distribution Statistics (IDS), come from Statistics Finland and are individual-level data. In these data the income information comes from tax and transfer registers. This data set is a repeated cross-section, a stratified random sample from a population of about 5 million Finns containing over 25,000 observations from about 10,000 households per year. On top of the cross-section there is a rotating panel in the data that follows one individual for two consecutive years.[6] The rest of the information is at the municipal level and has been linked to the IDS data. It comes from a survey of municipalities conducted by the University of Turku, a survey of municipalities conducted by the author, from the Social Insurance Institution of Finland and from Statistics Finland. Aggregated to the municipal level, the data constitute a panel where each municipality can be followed over the years.

The main estimation sample includes families whose youngest child is between 9 months and 3 years old. Pooled for all years, there are about

6 In the rotating panel each household is surveyed in two consecutive years and each year half of the sample consists of new households. Thus there are two consecutive observations for each individual.

6,000 households in this group and about 14,000 households that have children under the age of 6. The data contain a rich set of variables describing family characteristics, demographics, incomes and benefits derived from registers and surveys.

The main outcome variables are the employment status and earned income of a parent from tax registers and measured as a sum for the whole calendar year. I code a parent as employed if she or he has earned enough income per year, since I lack precise employment status in the data. Saez (2002) uses this kind of income-based measure and Eissa and Liebman (1996) define as employed all those who have positive working hours in a year. The employment threshold that I use is half of the annual mean income for women in the labour force in the data.[7] The alternative measure would be to use the number of months worked in the survey data. I do not use the survey data, since they are very noisy (there are cases where the total number of months worked in a year is over 12, and many report zero working months, although they earned income in that year). To demonstrate that there is a clear correlation with the constructed measure and a survey measure, Table 2 cross-tabulates the number of employed and non-employed individuals in the main estimation sample according to the two criteria. As expected, most mothers are classified in the same employment status using both variables, although there are some differences due to the errors in the survey data.

Table 2: Comparison of register- and survey-based employment measures

			Survey
		Not emp.	Emp.
Register	**Not emp.**	3,272	468
	Emp.	565	1,403

One could also use alternative definitions for the earnings thresholds. Table 10 in the Appendix presents the employment rates based on 50%, 30% and 70% thresholds. There the employment rate varies according to the chosen threshold in a predictable direction (higher threshold, more non-employed), but in all cases less than half of the main sample are employed. Moreover, the robustness section shows that the results are not overly sensitive to the precise selection of the employment threshold.

7 More specifically I measured income for women between 20 and 59 years old and not on sick leave, retired or otherwise outside of the labour force.

Table 3: Descriptive statistics for mothers

	Child 9 m.–2 years		Child 3–7 years		Treatment		Control	
	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd
Number of obs.	5,709		8,411		1,154		4,555	
Age	32.09	(5.26)	36.24	(5.40)	32.09	(5.06)	32.08	(5.31)
Earned income	7,726	(10,510)	14,983	(12,979)	8,477	(11,732)	7,536	(10,170)
Employment rate	0.35	(0.48)	0.65	(0.48)	0.34	(0.47)	0.35	(0.48)
Education N/A	0.11	(0.32)	0.12	(0.33)	0.11	(0.31)	0.12	(0.32)
Basic education	0.42	(0.49)	0.43	(0.50)	0.34	(0.47)	0.43	(0.50)
Higher education	0.26	(0.44)	0.26	(0.44)	0.26	(0.44)	0.26	(0.44)
Bachelor or higher	0.21	(0.41)	0.18	(0.39)	0.29	(0.45)	0.19	(0.39)
N of children <7 years	1.65	(0.70)	1.24	(0.45)	1.65	(0.69)	1.65	(0.71)

Notes: Mean and standard deviations of the descriptive statistics. In the columns "Child 9 m.–2 years" the youngest child is between 10 months and 2 years of age, in columns "Child 3–7 years" between 3 and 7 years of age, in the "Treatment" columns mothers are eligible for the supplement and in the "control" columns they are not eligible, and their youngest child is of eligible age.

Table 3 gives the descriptive statistics. It categorizes mothers according to the age of their youngest child in the left panel and the main estimation sample based on whether or not eligible for supplement in the right panel. The mothers in the main estimation sample, shown in the first column, are on average 32 years old and have at least high school education almost 50% of the time. The most typical families are those with one or two children, but there are also larger families in the data. Comparing the treatment and control groups, it seems that mothers in the two samples are very similar, for instance the average employment rates and ages are virtually the same. There are two notable exceptions: average earned income and the share with a higher educational degree are higher in municipalities providing the supplement.

The explanatory variable in the main estimations is the municipal supplement to the homecare allowance. The amount of supplement a mother is eligible for is imputed to everyone in the sample using their observable characteristics. The variables in the data used in the imputation include family size, the age of the children and residential municipality. The imputation matches the eligibility criteria with observations in the data. The eligibility criteria are described in Section 2. After the imputation the supplement variable states the amount in euros a mother is eligible for based on the observable characteristics. The variable is not conditional on the income or work status of the mother, since the supplement does not depend on income and the purpose of the variable is to

indicate the amount of supplement a mother is eligible for if she decides to take up the HCA and take care of her children herself. The main variation in the rules comes from a municipality implementing the supplement, changing the amount of the supplement and changing the age limit of the youngest child.

In the robustness checks I implement the supplement to parents of children older than actually eligible. This is mostly straightforward, but I need to simplify some minor rules related to the sibling extras. When the youngest child gets older, their siblings may already be of school age. Thus I simplify the rules used in the robustness checks by adding only an average sibling extra to the family if it has multiple children. In monetary terms these simplifications are minor, since sibling extras represent only a minor part of the supplement.

The homecare allowance and daycare fees depend on family income. For the income measure, I predict the income based on out-of-sample observations. The idea is to have a similar income measure for everyone. The problem is that the counterfactual income is never observed. More precisely, the income relevant for daycare fees is not observed for those not employed, and *vice versa* for the homecare allowance for those employed. Thus I need to impute income based on those parents with older children, and thus not eligible for the supplement. I take the income of those parents who have children older than 3 and are employed and match them in a linear regression to parents in the estimation sample (by conditioning on age, residential region and education). I then predict the income for parents in the main estimation sample from this matched sample of parents with older children. I do a similar process for non-employed parents. This procedure of predicting the counterfactual income based on predetermined characteristics follows the idea of Heckman (1974).

Having obtained these income measures, I impute the homecare allowance and daycare fees according to the national rules. These depend on family size and the age of the children in addition to family income. I assume that all the children are in homecare when a mother is not employed and that all the children are in public daycare when a mother is employed.

Also, I later estimate participation elasticity. That estimation utilizes income measures when employed and when non-employed. For the latter group I take into account the homecare allowance and other potential benefits. The difference in total income when employed and when not employed is the gain from participating in the labour force. I utilize the imputed income measures based on out-of-sample observations and predetermined characteristics, as explained above.

Table 4 shows how the supplement is spread over the years in the data and the mean value of the supplement conditional on being eligible for it. The table shows the mean values, standard deviations and the number of observations for

Table 4: Outcome and treatment variables

	All				Cond. on eligible			
Year	Supplement share	Empl. share	Income/ year	*N*	Supplement/ month	Empl. share	Income/ year	*N*
1995	0.06	0.36	6,305	761	120	0.46	7,881	46
	(0.24)	(0.48)	(8,294)		(30)	(0.50)	(9,754)	
1999	0.17	0.37	7,941	693	206	0.39	8,491	116
	(0.37)	(0.48)	(10,306)		(83)	(0.49)	(9,997)	
2001	0.20	0.39	8,932	713	212	0.35	7,216	145
	(0.40)	(0.49)	(10,554)		(76)	(0.48)	(8,906)	
2005	0.23	0.36	9,466	638	190	0.32	7,453	144
	(0.42)	(0.48)	(11,989)		(75)	(0.47)	(9,374)	

Notes: The left panel contains all observations for the selected years and the right panel only those who are eligible for the supplement in that year. The standard deviations are given within parentheses below the mean values.

supplement, employment rate and earned income. The table is for mothers whose youngest child is between 9 months and 3 years and is divided into two parts: the left panel is for all and the right panel for those eligible for the supplement. "Supplement" denotes the share of mothers eligible for the supplement. This figure has increased over time. The average monthly supplement was low at the beginning of the observation period, but is around 200 euros towards the end of the observation period. Mothers have a low participation rate and low earned income, but there do not seem to be systematic differences across the two panels. However, the employment rate (Empl.) and mean earned income (Income) have fallen, while the proportion receiving supplement has increased.

5 Estimation results

5.1 Main results

Table 5 shows the main estimation results. The dependent variables are the mothers' employment dummy and earned income. The monetary variables (such as the municipal supplement and earned income) are in 100 euros per month. I perform all the estimations for fathers as well and find a zero effect on their labour supply.

Table 5: The main estimation results

Outcome	Coefficient	(i)	(ii)
		DD	DDD
Work	Supplement	−0.033***	−0.036***
		(0.010)	(0.013)
	Obs.	5,709	11,205
	R^2	0.205	0.432
Income	Supplement	−939***	−1,108***
		(254)	(388.0)
	Obs.	5,725	11,287
	R^2	0.313	0.490
Second-level interactions		No	Yes

Notes: OLS estimates for a population of mothers. In the top panel, the dependent variable is the mothers' labour supply dummy. In the bottom panel, the dependent variable is earned income of mothers. The supplement is measured in 100 euros per month. Column (i) shows the DD results and column (ii) presents the DDD results. The third difference is between whether or not the youngest child is older than 3 years of age and the regression includes the second-level interactions, such as municipality by year fixed effects. Individual covariates used: age, education, spouse's income, number of children, the size of household and indicators for each 3-month intervals of the age of child. Municipal-level covariates used: municipal income tax rate, municipal unemployment rate, average income in municipality and the ratio of public-daycare places to the number of children in municipality. Robust standard errors in parentheses, ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

The results in Table 5 are organized as follows: there are two panels, top and bottom, which are divided according to the dependent variable. The top panel presents the results for the employment dummy, which is coded as 1 if the earned income is more than half of the annual average income of working-age women. In the bottom panel the dependent variable is earned income. The independent supplement variable is measured in 100 euros. Column (i) presents a DD estimate, including year and municipal-level dummies and other time-varying municipal-level control variables. The estimation includes individual-level covariates: dummies for the age of the youngest child for each 3-month age interval, dummies for each sibling, mother's age and spouse's income. The municipal-level covariates include the municipal income tax rate, municipal unemployment rate, average income in the municipality and the ratio of public-daycare places to the number of children in the municipality. The results are robust to including or excluding any one or all of these covariates. Column (ii) presents the DDD results. The third difference is between having the

youngest child in the age group of 9 months to 2 years or 3–5 years. The latter age group is never entitled to a supplement or the homecare allowance. An advantage of the DDD estimate is that it makes it possible to control for very flexible municipality-specific time trends.

The main result for the employment dummy indicates that increasing the municipal supplement by 100 euros per month causes 3 percentage points fewer mothers to participate, which is almost 10% of the mean participation rate. The main result for earned income indicates that increasing the municipal supplement by 100 euros per month decreases annual earned income by 1,100 euros.[8] The magnitude of the income effect is consistent with the employment rate increasing by 10% and mothers moving into full-time employment. The mothers' average full-time annual earned income (net of taxes) during this period was less than 20,000 euros. Since there is variation in the way in which mothers respond to the municipal supplement, I interpret these results as the average treatment effect on the treated.

5.2 Robustness and sensitivity checks

To check against spurious specification in the main estimations, I perform robustness checks. First I estimate whether mothers' employment and earned income differ between the treatment and control groups already prior to changes in municipal supplements. More specifically, I take leads and lags from a DD variable, eligibility for P euros of supplement. The top panel of Figures 3 and 4 presents estimated coefficients for these leads and lags.[9] The coefficients of the lags are zero, whereas the current year estimate and first lead are negative and statistically significant. The zero result from the coefficients of the lagged policy variables indicates that the policy changes were not implemented due to changes in maternal employment and there were no anticipation effects. It seems that the treatment and control groups behaved in a similar way prior to reforms in supplement policies. Thus the data seem to behave according to the assumptions in the natural experimental approach.

8 The average net-of-tax income per month for a woman working full time is around 1,500 (own calculations).

9 Some of the municipal supplement rules were simplified in order to be able to calculate the leads and lags, like removing the age restrictions of the sibling extras to the supplement. These simplifications do not affect the euro amounts of the supplement much and, more importantly, do not delete or create any reforms to supplement policies. The regression included municipal and year dummies, as well as dummies for every 2 years of age of the youngest child and controls for characteristics of the mother.

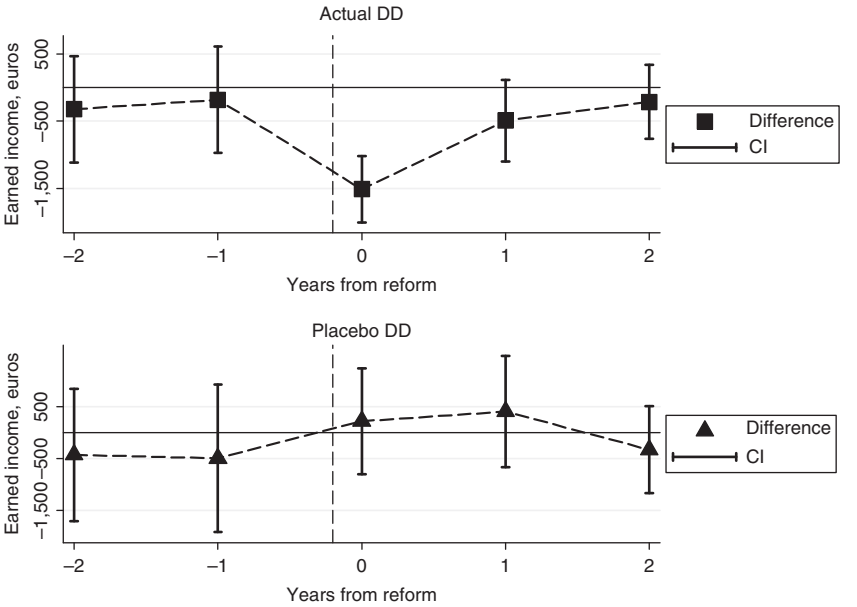

Figure 3: Visual presentation of DD approach to mothers' earned income
Notes: Top panel presents the lags and leads of the DD coefficient for mothers whose youngest child is between 9 months and 3 years old from a regression that is similar to the main estimates. The bottom panel presents similar coefficients performed on the main placebo group: mothers whose youngest child is between 3 and 5 years old.

Families with older children appear natural candidates for performing a robustness check, since their characteristics, apart from the age, should be close to families in the actual treatment group. Corresponding to this idea, the bottom panels of Figures 3 and 4 present the coefficients of the leads and lags of a placebo DD variable. The variable makes mothers whose youngest child is between 3 and 5 years old eligible for the municipal supplement if they live in a supplement municipality. It is reassuring for the identification that the coefficients of the leads and lags, as well as of the current year, are all statistically not different from zero. This builds confidence that the identification strategy used in the main estimates reveals a causal relationship between supplement policy and maternal labour supply.

I perform further robustness checks in Table 6. Column (i) presents a DD estimate for the placebo variable for mothers with children 3–5 years old. The estimates are otherwise similar to those in Table 5, column (i) for the employment dummy outcome. This placebo estimate also produces a non-significant result.

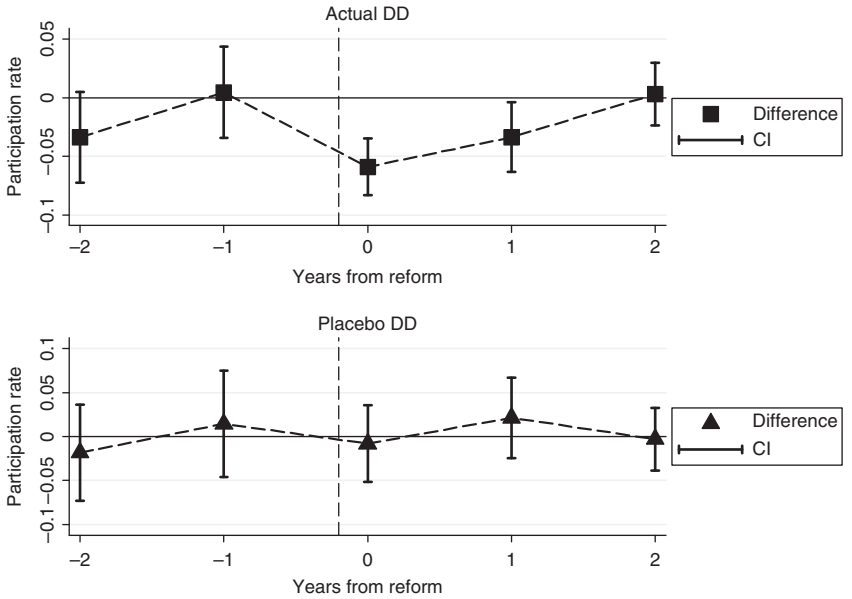

Figure 4: Visual presentation of DD approach to mothers' participation
Notes: Top panel presents the lags and leads of the DD coefficient for mothers whose youngest child is between 9 months and 3 years old from a regression that is similar to the main estimates. The bottom panel presents similar coefficients performed on the main placebo group: mothers whose youngest child is between 3 and 5 years old.

Column (ii) of Table 6 presents a robustness check for a different group: women who are going to have a child the following year. Here I utilize the rotating panel feature of the data. The rotating panel follows half of the sample to the next year. Thus the rotating sample is only half of the total data. The model in column (ii) is estimated for families that will have a child aged 9 months or younger the following year, but do not currently have any children between 9 months and 2 years old. The supplement variable is the value of supplement a family will be eligible for in 2 years' time. This estimate should tell something about the potential anticipation effect. However, the coefficient of the supplement is zero, indicating no anticipation. This indicates that there is no serious anticipation effect (although the result is imprecise, since the sample size is only 541 due to the strong sample restrictions).

Column (iii) of Table 6 presents a robustness check against fertility and residential sorting. The dependent variable is a dummy indicating whether or not being a child under 3 years old. The regression is otherwise similar to the main estimates, except for the outcome, and it is estimated for everyone in the

Table 6: Robustness checks

	(i)	(ii)	(iii)	(iv)
	DD 3–5 years	**DD next year**	**DD prob.**	**DD main**
Supplement	0.0129	0.0149	−2.83e−06	−0.0316***
	(0.0136)	(0.0509)	(1.41e−05)	(0.0101)
Obs.	4,722	541	217,837	5,877
R^2	0.219	0.548	0.057	0.259
Years	Yes	Yes	Yes	Yes
Municipalities	Yes	Yes	Yes	Yes

Notes: Standard errors in parentheses (clustered on municipal level). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. The dependent variable in columns (i), (ii) and (iv) is the mother's labour supply dummy and in column (iii) it is an indicator with the value of 1 for children under the age of 3 as a dependent variable. The estimation sample in column (i) is mothers whose youngest child is between 3 and 5 years old and in column (ii) women who will have a child next year. Column (iii) is estimated for everyone in the data. Column (iv) shows the main result, but with simplified supplement rules.

data. The supplement variable in this regression has a positive coefficient if the number of children in the treatment group increases at the same time as there are increases in the amount of supplement a family is eligible for. The result shows that changes in supplement eligibility rules do not have any effect on being a child under the age of 3. Thus families with small children do not move to municipalities that change their supplement policies in a more generous direction.[10] Moreover, mothers in supplement municipalities do not seem to be more fertile because of changes in the supplement.

Column (iv) presents a baseline estimation, but with the simplified rules needed for the implementation of a supplement used in other robustness checks. These simplifications mainly removed age thresholds from sibling extras, were minor in monetary terms and did not create any new changes to supplement policies. The coefficient of the municipal supplement is similar to the main estimates, and the simplification of rules does not seem to affect the estimates.

As a further sensitivity check, I added a linear time trend for every municipality, used another definition for the employment dummy[11] and excluded

10 Finnish municipalities are typically large in surface area. Thus moving to another municipality usually means moving to a completely different city or town.

11 The other definition is the number of months worked as based on a survey question. The results for this are shown in Table 11. There is a measurement error in this variable, thus I did not use it in the main estimates.

single municipalities or years, without any significant effect on the results. I also estimated the DD model with a different earnings threshold (30%) for defining a parent as employed. Tables 12 and 13 show some of the results.

In summary, the main result in column (ii) of Table 5 seems to be robust to a fairly flexible set of control variables. In effect, once the municipal and year fixed effects are included, the coefficients of the eligibility for P euros of supplement is robust to either including or excluding covariates like mother's or spouse's age or education, spouse's income, municipal unemployment or demographic characteristics and even year by municipality fixed effects. However, it is necessary to control for the age of the youngest child. Since this variable is closely correlated with the employment of mothers, and the treatment is not constant within the age of the youngest child, it turns out to be important to include this covariate, and this is done in all the applicable estimates.

5.3 Divided sample results

To check for possible variation by sub-groups, I divided the sample according to the mother's education. Education is a predetermined characteristic. Thus dividing the sample along it should not be correlated with treatment effects. Table 7 collects the results. The dependent variable is the mothers' employment dummy in the top panel and earned income in the bottom panel. The supplement is measured in 100 euros per month.

Table 7: Divided sample results for education

		Low ed.	Medium ed.	High ed.
Work	Supplement	−0.075**	−0.019	−0.05**
		(0.034)	(0.014)	(0.023)
	Obs.	647	3,873	1,189
	R^2	0.462	0.226	0.380
Income	Supplement	−773	−269	−2,288***
		(602)	(234)	(610)
	Obs.	647	3,888	1,190
	R^2	0.498	0.241	0.392

Notes: Standard errors in parentheses (clustered on municipal level). ***p < 0.01, **p < 0.05, *p < 0.1. The dependent variable in the top panel is the mothers' labour supply dummy and in the bottom panel mothers' earned income. The sample of mothers whose youngest child is between 10 months and 3 years old is divided by mother's education. All the estimates were controlled with the same control vector as for the main results, and it includes spouse's earned income.

The divided sample results by education suggest there is a U-shape in the response profile: low- and high-educated mothers respond more than medium-educated mothers. The same overall result is obtained for both dependent variables, although the earned income coefficient is not statistically significant for the low-educated group.[12] It is intriguing that for the high-education group the coefficient is larger than for the medium-education group. This is interesting because education is usually highly positively correlated with income and one would think that the participation response decreases with income (see e.g. Saez 2002). The result suggests that mothers with high-income potential respond more to changes in taxation than mothers with medium-income potential.

The divided sample result is interesting, because it could reflect the fact that the total amount of HCA is high enough for relatively high-earning mothers to find it attractive to stay outside of the labour force for many years. If the total HCA level was much lower, as in Sweden, it would be likely that only low-income mothers would respond to it. However, it is also possible that the U-shape result by education has something to do with educational attainment directly and not the apparent correlation with education and income. I cannot test against this directly, since I do not observe mothers' income prior to having children. Finally, all the results are controlled for spouse's income. Therefore the explanation for the result is not that more highly educated mothers have wealthier spouses.

In the previous literature, there are a few attempts to estimate labour supply responses across income or education groups (Eissa 1995 and Eissa and Liebman 1996). These estimates give evidence that higher-income taxpayers could be responsive in other settings as well.

6 Participation elasticity

This section provides the participation elasticity estimates. The reduced-form estimates show the effect of a given amount of the supplement on participation, whereas the participation elasticity calculated here takes into account the financial gain from participation for everyone. In doing that, it explains how the financial gain affects participation. It also produces a more precise and externally valid measure than a mere back-of-the-envelope calculation using the reduced-form estimate. The differences in financial gain from participation may explain differences in participation rates across different institutional settings in different countries. The participation elasticity is a key parameter in

12 For participation essentially the same set of results emerged when the participation threshold was defined as 30% of the mean income of the education group.

policy evaluation needed for evaluating the optimality of tax systems (Mirrlees Review 2011).

Normally the fundamental problem in estimating the participation elasticity is that the counterfactual income corresponding to the counterfactual participation state is not observed. To solve for this, I estimate the counterfactual income for everyone following the literature started by Heckman (1974). I then use the exogenous variation in the participation income created by changes in the (non-income-dependent) municipal supplements to estimate the effect of participation income on participation decisions.

In estimating the counterfactual income I impute two income measures for everyone. The first measures the after-tax income of a non-employed mother (who takes care of her children at home and also receives homecare benefits) and the second measures the after-tax income of an employed mother (who puts her children in public daycare). The gain from participating, the net participation income, is calculated by taking the difference of these two measures. The process of imputing income is explained more carefully in Section 4.

Despite being estimated based on predetermined characteristics, we expect the participation income to be endogenous to participation status. To solve for this, I use the municipal supplement as an instrument for changes in participation income. The various robustness checks presented in the previous section contribute to validating the exogeneity of this instrument. It is also important that eligibility for a certain amount of supplement does not depend on family income and that one actually receives it only when not participating. For the latter reason it affects the financial gain from participating.

The results are shown in Table 8. The explanatory variable is a dummy for participation status as in the main estimates. Now the unit of measurement is 1 euro per year. The first stage, shown in the first line, is very strong. There is almost a one-to-one relationship between participation income and the amount of supplement one is eligible for. The second-stage result (2SLS) implies that 1 additional euro in participation income leads to an increase in participation probability of 0.003 percentage points.

The participation elasticity (η) implied by the coefficient in Table 8 can be calculated as

$$\eta = \frac{dparticipation}{dincome} * \frac{income}{participation} = \beta * \frac{income}{participation}$$
$$= 0.0000295((9,814)/(0.348)) = 0.83$$

where β refers to the coefficient of participation income in the second-stage regression. The above elasticity is calculated on an annual basis. The figure of 9,814 euros, *income*, is the average change in net income associated with labour

Table 8: Change in mother's participation in response to changes in income

	Participation	*F*-value
First stage	−1.037***	925.44
	(0.0341)	
Change in incomes	−2.95*e*−05***	
	(8.85*e*−06)	
Obs.	5,876	
R^2	0.191	
Elasticity	0.83	

Notes: Standard errors in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. 2SLS results for the mothers' participation dummy. The first stage regresses the municipal supplement on change in income associated with entry. The second stage explains the participation dummy with the first-stage predicted value.

market entry for a typical mother. The figure of 0.348, *participation*, is the average participation rate in the population for which the estimation was made.

Table 9 shows the participation effect by education level. For mothers with low or high education the result is higher than the average. The participation

Table 9: Participation response by mother's education

	(i)	(ii)	(iii)
	Low educ.	Med. educ.	High educ.
First stage	−1.081***	−1.037***	−1.113***
	(0.103)	(0.026)	(0.042)
F-value	110.8	1,650	706.5
Change in incomes	−3.78*e*−05*	−1.72*e*−05	−3.81*e*−05***
	(2.16*e*−05)	(1.12*e*−05)	(1.48*e*−05)
Obs.	696	3,977	1,203
R^2	0.439	0.212	0.357
Elasticity	1.3	0.4	1.5

Notes: Standard errors in parentheses (clustered on municipal level). ***p < 0.01, **p < 0.05, *p < 0.1. 2SLS results for the mothers' participation dummy divided by their education level. Column (i) shows the results for mothers with basic education or for whom education information was not available, in column (ii) the highest education attained is high school or equivalent and in column (iii) bachelor level or higher. Change in incomes and other monetary values measured in euros per year.

elasticities implied by these estimates for low, medium and high education levels are $0.000038 * (7,050/0.2) = 1.3$, $0.000017 * (8,060/0.35) = 0.4$ and $0.000038 * (17,470/0.44) = 1.5$, respectively. These participation elasticities confirm the U-shape profile of the participation elasticities by education suggested by the divided sample results. More highly educated mothers respond to financial incentives more than medium-educated, even when accounting for their above-average income.

The average participation elasticity estimated here is large, but within the bounds of the elasticities derived in earlier literature. To report some earlier estimates in the literature, Baker, Gruber, and Milligan (2008) estimated a participation elasticity of 0.236 resulting from decreasing childcare cost, and Milligan and Stabile (2007) reported an elasticity of 0.96 for having earnings as a major source of income utilizing the Canadian benefit reform. Eissa and Liebman (1996) estimated a participation elasticity of 0.6 for single mothers.

7 Conclusions

This paper presents evidence on the extent to which child homecare benefits affect parental labour supply. An important component of this study is a particular feature of the Finnish childcare system: a municipal supplement to the HCA. This provides plausibly exogenous variation in the labour supply incentives of mothers. Since the HCA is related to homecare, it reduces labour supply incentives. The financial support for homecare is sizeable: the average HCA is between 300 and 700 euros per month and the extent of non-income-related supplements is from zero to over 200 euros per month.

I found a very robust result that the HCA has a negative effect on maternal labour supply. The main estimate indicated that increasing the municipal supplement by 100 euros per month causes 3 percentage points fewer women to participate. However, I did not find any effect on the labour supply of fathers, who are also eligible for the HCA. Furthermore, the results suggest that when the HCA period ends, mothers return to employment.

The advantage of analysing the HCA is that the supplement does not depend on income. The alternatives are daycare fees or means-tested benefits that depend on income. Income-related benefits typically depend on an income level that is associated with unobserved ability, which leads to a potential endogeneity problem. On the contrary, this problem does not apply to changes

in the municipal supplement, which only depends on predetermined characteristics like the age of the youngest child.

Utilizing this feature, I put the results into a broader policy context by estimating the participation elasticity. This measures to what extent the individual financial gain from a participation decision causes parents to participate. The estimation uses the exogenous variation in participation income created by the municipal supplements as its instruments. The result implied a participation elasticity of around 0.8. This estimate is towards the high end of the range of elasticities found in other studies (Baker and Milligan 2008; Lefebvre and Merrigan 2008 and Milligan and Stabile 2007). The large participation elasticity indicates that targeting sizeable incentives against labour market participation may substantially reduce the participation rate. Thus the overall economic consequences of the HCA are quite significant.

When studying labour supply decisions, it is important to take the whole benefit system affecting any participation decision into account. Other papers analysing Nordic childcare institutions found a small effect on maternal employment (Simonsen 2010; Havnes and Mogstad 2011 and Lundin, Mörk, and Öckert 2008). It is possible that the marginal effect of a childcare subsidy is smaller if the underlying benefit system is such that non-employment is not an attractive alternative for parents, or if the employment incentives are increased when the participation rate is already very high. Moreover, there are indications that targeting benefits at homecare may have stronger labour supply implications than targeting benefits at other childcare (Schone 2004; Naz 2004 and Gathmann and Sass 2012).

Appendix

Further robustness and sensitivity checks

Employment rates in the main estimation sample for mothers based on different income thresholds

Table 10: Maternal employment rates according to different criteria

Definition	Rate, %	Sd	*N*
Main (50%)	0.34	*0.48*	5,709
Low (30%)	0.44	*0.50*	5,709
High (70%)	0.27	*0.44*	5,709

Table 11 reports estimates based on an outcome variable where mothers have reported working 10 or more months in an interview. Column (iii) reports a DD estimate equivalent to the main estimates. Although the coefficient is smaller, it implies a similar participation elasticity, since the participation elasticity measured in this way is smaller than that in normal estimates.

Table 11: Estimations with work dummy based on survey question

	(i)	(ii)	(iii)
Supplement	−0.055***	−0.084***	−0.014*
	(0.0062)	(0.0096)	(0.0083)
Years	No	Yes	Yes
Municipalities	No	Yes	Yes
Child age		No	Yes
Obs.	6,023	6,023	6,023
R^2	0.013	0.139	0.273

Notes: Standard errors in parentheses (clustered on municipal level). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. OLS estimates for the mothers' labour supply dummy with a value of 1 when reported working 10 or more months per year in a survey. Supplement measured in 100 euros per month.

Table 12: Sensitivity checks

Outcome	Var.	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Participation	Supplement	−0.026*	−0.042**	−0.026*	−0.030**	−0.036***	−0.027***
		(0.013)	(0.021)	(0.013)	(0.013)	(0.0094)	(0.010)
	Obs.	5,709	3,572	4,493	5,273	5,709	5,709
	R^2	0.225	0.245	0.223	0.206	0.206	0.203
Income	Supplement	−974***	−1,133***	−861**	−693***		
		(342)	(423)	(361)	(233)		
	Obs.	5,725	3,581	4,506	5,289		
	R^2	0.325	0.350	0.321	0.313		

Notes: Standard errors in parentheses (clustered on municipal level). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. OLS estimates for the mothers' labour supply dummy and earned income. Supplement measured in 100 euros per month. Column (i) includes linear municipal trends where some smaller municipalities were grouped together. Column (ii) includes only the years from 1995 to 2001 and column (iii) only the years from 1998 to 2005. Column (iv) leaves out the largest municipality in the sample. Column (v) sets the employment threshold at 70% of the mean income, and column (vi) sets the employment threshold at 30% of the mean income.

Table 13: Sensitivity check: results for fathers

Outcome	Coefficient	(i)	(ii)
Work	Supplement	0.0026	0.0043
		(0.0093)	(0.0095)
	Obs.	5,527	5,527
	R^2	0.197	0.224
Income	Supplement	244	–91
		(279)	(262)
	Obs.	5,560	5,560
	R^2	0.366	0.387

Notes: Standard errors in parentheses (clustered on municipal level). ***p < 0.01, **p < 0.05, *p < 0.1. OLS estimates for fathers' labour supply dummy and earned income. Supplement measured in 100 euros per month. Column (i) is similar to the main DD estimates for mothers. Column (ii) adds linear municipality trends to this.

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